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SURGICAL SHOCK AND SOME RELATED PROBLEMS.[1]

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The subject of surgical shock has always been of deep interest to me, not alone because it is one of the unsolved problems of surgery, but because it affords to my mind such a striking example of one of the weak points in our present system of medical teaching. I mean the tendency to dislocate physiology from clinical medicine, the tendency to look upon physiology as a preparation for and not a part of. If the study of shock has taught anything, it is that the subject must be approached either by the physiologist who knows his clinical medicine or by the clinician who knows his physiology, and that physiology must be taught and learned more as a factor of the every-day practice of medicine and surgery. Anatomy is the foundation-stone of disturbed form. Pathology is in small part anatomy gone wrong; in major part it is only physiology gone wrong.

Therefore I like to talk of shock if for nothing else than to discourage the would-be surgeon. The greatest problem of modern surgery is how to make better surgeons. We may well begin by holding up before the youthful aspirants—every medical student more or less seriously contemplates the surgical career, the one and only ideal—a surgeon must know as much as any man in any branch of medicine.

[1] Read before the Medical and Chirurgical Faculty of Maryland, Baltimore, Maryland.

The first and chief trouble with the shock problem lies in the fact that we have no good definition. Shock is defined as a bleeding to death in the venous system of the body, that is, that in shock the blood collects in the veins, chiefly of the splanchnic system, and the patient dies because the blood fails to reach the right heart. But this disposition of the blood in shock is but a symptom, and but a symptom common to many other conditions which are not shock. It is like defining malaria as a febrile disease, characterized by regular remissions and exacerbations of temperature. Now that is what the malarial parasite does to the body, as it follows out its life cycle, but the fever is not malaria, nor is the peculiar wave-like fever malaria. The infection with this certain bug is malaria. So a definition of shock, as a bleeding to death in the veins, is only a definition of a symptom, not of a cause. A more accurate definition is, however, impossible, since we do not know the cause, therefore the next necessary step is to define and limit the definition, with the consequent result that inaccuracies, misstatements and confusions reign.

Let us agree that shock has something to do with blood-pressure and consider for a moment what blood-pressure is:

Three elements are united in the expression "blood-pressure:"

1. The pressure due to the specific gravity of the fluid itself—hydrostatic pressure.

2. The pressure caused by force exerted upon this fluid by the vessel walls and other external factors, the fluid being contained in a closed system of elastic tubes—hydraulic pressure.

3. The pressure due to movements of the fluid itself—hydrodynamic pressure.

Of these three elements the third, hydrodynamic pressure, hardly enters into a consideration of what is ordinarily included under the term blood-pressure. It is of importance in the study of certain manifestations of the transmission of pressure, such as the study of the pulse wave, but for our present purpose this hydrodynamic pressure can be left aside.

A consideration of the hydrostatic pressure of the blood leads us at once to a realization of the complexity of the factors which govern the pressure of the fluid within the blood-vascular system. It is evident that the pressure of every layer of blood must be increased by the weight of that amount of blood which lies in a vertical direction upon it, that is, considering now only the purely physical conditions and leaving all the other factors out of account. Thus the pressure of the blood in the arteries of the adult foot must be increased by the weight of a column of 165 cm. of blood (equal to 13 cm. of mercury), entirely apart from all other factors. The hydrostatic pressure in the veins of the foot and the leg is of surgical interest in the consideration of varicose veins and varicose ulcers, because varicosities are a question of hydrostatics, plus,

of course, some abnormal quality in the vein which makes them susceptible to this factor. A person with varicose veins finds relief on raising the foot to the level of the body. The varicosities appear chiefly in the veins of the subcutaneous tissue, where the veins have only the external support of this tissue. Therefore we can surgically treat and cure the condition by simply ligating these subcutaneous veins, thus forcing the blood to return by the deeper veins which lie in and beneath the muscles, and whose thin walls are strengthened by the structures which surround them on all sides.

Now normally this hydrostatic pressure is of little practical importance, since compensating devices are so well in hand, but in abnormal conditions surgery has often to think of pure hydrostatic pressure. One of the most instructive examples of the effect of hydrostatic pressure acting alone is the experiment of Hill and Barnard. If a tame rabbit be held in the vertical, head-up position, the blood will collect in the lower areas, especially the splanchnic area, and in a comparatively short time so much of the blood of the body may accumulate in this area as to cause the death of the animal from anemia of the brain. The rabbit has bled to death into the vessels of its own splanchnic area, yet we can hardly say it died of shock, for if we did our definition of shock would therefore have to be, shock is a lack of tone of the anterior belly wall; because if the experiment be tried with a wild rabbit whose abdominal muscles are strongly developed, or if the belly wall of the tame rabbit be supported by a bandage, the animal does not die.

This experiment illustrates an important point in the study of the pressure of the blood in the venous system, the enormous, almost determining factor, played by the muscles in the support of the vein walls. We must indeed look upon the body muscles functionally as a very important element of the walls of the veins themselves. It is this factor which must be considered surgically in the case of patients whose muscular system is relaxed from a long illness in bed. If they rise too quickly to a sitting position they may faint. A patient who has fainted is not to be placed in a sitting position but laid flat so that the hydrostatic pressure does not cause the blood to collect in the veins whose muscular walls, in this sense, are relaxed. A distended urinary bladder, a large cyst of the ovary, must not be emptied too rapidly or else an essential vacuum will be created into which the blood will run simply because of hydrostatic pressure.

The role played by hydrostatic pressure in the condition of shock is not clear, but it is a factor which cannot be ignored. It may indeed be that the blood collects in the veins of the splanchnic area simply because of hydrostatics; these veins are certainly the ones which have the least external support from the surrounding structures.

The hydraulic pressure of the blood is the result of a primary force produced by the contraction of the heart, acting against a resistance which is the result of the friction of a viscid fluid, chiefly in the small vessels of the periphery. What we commonly call blood-pressure is therefore the tension of the vessel wall produced by the systole of the ventricle and transmitted by an incompressible fluid. This tension of the vessel wall will depend upon the elasticity of the vessel wall and the force which stretches it. The elasticity depends upon: (1) The inherent elasticity, due to the presence of elastic membranes and fibers. Therefore the high pressure of the hardened arteries of advancing years is due to the fact that the force of the heart beat is carried straight to the periphery, not gradually taken up and absorbed by the expansion of an elastic tube. (2) The "tone" of the involuntary muscle cells, which cells persist in the smaller arteries and the arterioles, after the elastica has disappeared, and which form the most important structural element in the vessel wall of these smaller vessels. "Tone" is a term applied to that condition of the muscle cells which results in their being always stretched. If you cut any muscle the ends draw apart. This is a simple explanation of what the physiologists term "myotatic irritability," or the responsivity to the mechanical stimulus of stretching. (3) This "tone" depends upon nerve impulses and upon the presence in the circulating blood of the secretions of certain internal glands. The force acting to stretch the vessel wall will depend upon (4) The amount of blood in the vessel, which is in turn dependent upon the relation of the inflow to the outflow. The inflow will depend upon (5) the volume of blood forced out of the heart at each systole and (6) the rate of the heart beat. The outflow will depend upon the resistance, especially (7) the resistance in the arterioles and (8) the consistency of the blood. Under abnormal conditions we must also, as was just pointed out, consider the influence of (9) the hydrostatic pressure. In the background of it all there looms the omnipotent activity of (10) the vasomotor centers.

We might then define blood-pressure as a constant resulting from the summation of these ten variables, a concept which would naturally result in anything but a constant. I have mentioned these factors of blood-pressure not to impress you with the extent of my erudition, but to offer you perhaps the best explanation of why there are so many theories regarding shock. Since we do not know the cause we cannot determine upon which factor the causative agent works, and so, as in all fields of medicine where knowledge is lacking, a man will incline to the theory which fits best with his own mental process, or with the particular line of work in which he may be interested.

Thus we have Henderson, a physiologist whose chief interest is the study of the gas exchange of the body, including, of course,

the blood gases and the final interchange of gases between the tissue and blood. The importance of the exciting effect of carbon dioxide upon the vasomotor center stands out to Henderson as the one important factor, and shock is therefore a result of what he has termed acapnia, which, being interpreted, is the condition of there being not enough carbon dioxide in the blood to properly stimulate the vasomotor centers. Therefore the pressure falls. Since the carbon dioxide is a determining factor in the total acidity of the blood we next come into close personal contact with hydrogen ion concentration, and so on into the realms beyond the grasp of the simple surgical mind. The facts of carbon dioxide stimulation of the center of the vasomotor system are easily demonstrable. The opposite of this fact, a condition in which there is not enough carbon dioxide, I have never seen in my experiments, nor do I attach much weight to this theory of shock. Perhaps because I was born and raised a Presbyterian I have an abiding faith in the idea that things are arranged to meet certain ends. The breath of life is so fundamentally essential that I feel that when it fails we die, and die quickly, not by the long-drawn-out process characteristic of shock. I realize that this line of reasoning would not long stand the penetrating searchlight of serious questioning, and I trust you will take my theology simply for what it may be worth.

Now, Meltzer is a physiologist of long years' experience. He has always been deeply impressed with the forces which regulate and control activity. With every impulse to do there comes another impulse not to do; with stimulation comes inhibition. This is perhaps best seen in the phenomena of alcoholic saturation of the body. Men do things under this pleasing stimulus not so much because of the stimulation as because of the failure of normal inhibition. There is a certain type of acute failure of the circulation doubtless referable to inhibition, seen after a blow on the chin or the larynx or the solar plexus. That normal balance between stimulation and inhibition which results in the normal heart beat is lost because of a disturbance of the inhibiting mechanism—the heart fails. This theory has this much to commend it—it certainly accounts for certain conditions which are characterized by low blood-pressure, but it as certainly does not account for all the conditions commonly called shock.

The laboratory side is not alone in inclining to an explanation of shock according to the varying ideas and ideals of the individual. The clinician has, to my mind, introduced a great element of confusion into the whole subject, not by advancing too numerous ideas as to the theory of the thing but by his tendency to herd all fatal cases in surgery into the group of shock deaths. There is perhaps a psychological reason for this. It is doubtless a greater comfort to the relatives of the deceased to feel that their beloved was taken by some mysterious act of Providence, even perhaps a

certain pride that a case of such unknown nature should have occurred in their family, than it would be to feel that the patient was called to his reward as the result of the slipping of a catgut ligature, or the slipping of the surgeon's foot just as he was pulling the kidney out of the lumbar incision.

Now, if you will recall that I mentioned ten factors which entered into blood-pressure, a disturbance of any one of which might conceivably result in death with low blood-pressure, you will understand why I feel that it would add to our understanding of the problem, especially as regards practice, if we would separate the clinical conditions, which may be followed by low blood-pressure and consequent death into four groups:

1. I would define as syncope—from συγκοπή, a cutting short—the condition in which the blood-pressure falls because the great automatic centers are cut off short. A blow on the head may result in immediate syncope (cutting short) if the force be sufficient to cause that condition of the cerebral centers best spoken of as *commotio cerebri*, or else a cerebral edema or a hemorrhage follows and the intracranial pressure rises until it cuts short the function of the centers. The bounding pulse characteristic of the early stages of this condition is sufficient proof that the vasomotor mechanism is performing its function. It is obvious from the practical side that in such a condition the head should not be lowered or the pressure in the cranium would be merely increased by adding the factor of gravity in the venous system to an already dangerous pressure; nor would bandaging of the limbs, nor stimulation of the vasomotors, nor infusion of salt solution be indicated. The only direct indication is to relieve this pressure on the centers.

2. I would define as collapse—*collabere*, to fall together—the condition in which by the phenomenon called cardiac inhibition the machinery of the circulation fails—the heart falls to pieces, as it were. This is the condition explained by inhibition. Here again, vasomotor stimulation is not indicated; saline infusion, bandaging of the extremities, perfusion of blood—all this is contra-indicated; the engine is stalled, and putting on more load will not start it. In fact it is not unlikely that a venesection would be more of help in this condition than any effort which would increase the burden of the heart; the poor thing, remember, is having troubles of its own.

In the third place I would put hemorrhage. Without denying that loss of blood may be a factor in the production of the condition which appeals to my mind as true surgical shock I still object to grouping deaths due to hemorrhage under the caption of deaths due to shock. Death occurs after hemorrhage because the patient bleeds to death, and I would suggest that we forget that shock has anything to do with it.

Now, having ruled out head death, heart death, hemorrhage

death, what is left to constitute the group which is true surgical shock? Obviously the problem has lost in importance. True surgical shock is to my mind the condition marked by a gradual, persistent, progressive fall of blood-pressure such as characterizes certain cases after extensive crush injury with practically no loss of blood, cases of extensive burn, and many intra-abdominal conditions, particularly high intestinal obstruction, or ileus, and acute hemorrhagic pancreatitis. These are in my opinion instances of primary failure of the peripheral vasomotor mechanism, and I further believe that they are associated in some manner with a disturbance of adrenal function. Remember that the point where the vasomotor system accomplishes its work, transforms the energy of the centers into work, is not in the venous system, nor in the capillaries, but in the arterioles, and the mechanism which transforms energy into work is the musculature of the arterioles. We know that when the adrenals have been largely destroyed by disease we have the picture of Addison's disease, a picture characterized by the asthenia of the skeletal muscles and also by the asthenia of the musculature of the arterioles, hence the consequent low blood-pressure. We know that this true form of surgical shock is especially apt to follow a manifest toxemia, as after severe burns or in hemorrhagic pancreatitis. We know that the adrenals are almost specifically affected by some toxins, as in diphtheria, and we know that the prolonged administration of chloroform or ether certainly injures the adrenals. Further, the only way in which I have succeeded in producing experimentally the condition which to me compares with surgical shock is by the complete removal of both adrenals. From the practical side we know that adrenalin, since Crile's introduction of the drug in practice, has proved valuable in the treatment of this condition.

Now, it is my belief that adrenalin produces a good effect not only because it raises the blood-pressure, for which purpose alone it was introduced, but because it supplies a something which is certainly essential to life, and in these cases is apparently lacking. The treatment of surgical shock must therefore consist in the continued administration of adrenalin plus efforts to remove the causative factor. In cases due to a manifest toxemia it is certainly only symptomatic treatment to endeavor to sustain blood-pressure and not make an effort to remove the primary toxin. This is illustrated by Hartwell and Hoguet's experiments with dogs which otherwise would have died from high intestinal obstruction in three days, but which were kept alive for ten days or longer by the use of large amounts of saline solution, probably because the toxin responsible for the death of the animal is thereby more readily eliminated. I suspect that the problem of which vasomotor stimulant to choose, whether caffein or strychnin, etc., is probably a simple one—they are all aimed at the wrong end of the vasomotor system. Not

the vasomotor centers, but the mechanism at the periphery which transforms the energy of the vasomotor centers into work, needs stimulation; hence the good results of bandaging the extremities and using the pneumatic suit, which are mechanical supporters of the periphery, and hence the good of adrenalin, the chemical supporter of the periphery. This problem of surgical shock is primarily a clinical problem, and ought to be approached by the practical man who has not forgotten his theory.

You may have begun to wonder why I have ventured an attempt at clarifying a problem by the apparent method of making it more complex. I have done it from the stand-point of practice, for it is very clear to me that not all conditions of low pressure or shock should be treated by the same method. A patient coming with a head injury, or with an inhibited heart, or with internal hemorrhage, or with high obstruction, should not, I feel, be classed together from the stand-point of treatment. The head injury needs relief from the intracranial pressure; the heart case needs a heart stimulant and perhaps a venesection rather than transfusion; the hemorrhage case needs fluid in the vascular system, but not necessarily vasomotor stimulation; the high obstruction needs, first, removal of the toxin which is killing him, therefore washing out of the stomach and the duodenum if that is possible; washing out of the vascular system by saline solution, for the poison is in the blood; stimulation of the peripheral vasomotor apparatus, because it, too, is poisoned.

I have not as yet mentioned the doctrine of anoci-association. This is a remarkable teaching because it finds no basis in the known facts of physiology, yet it has accomplished far-reaching results in practice. The word is made up from alpha-privitive, the root of the word noxious or harmful, and association, and means the removal of all harmful impulses. The physiologist teaches that there is a constant stream of impulses from the periphery to the centers. Crile teaches that these impulses can in time wear out or fatigue the vasomotor centers and then we have shock. These external harmful impulses are especially numerous in a modern hospital. Crile holds that anoci-association begins with the doorman, and anyone who has seen the superb insolence of the average doorman will understand this point. This is undoubtedly the good of this doctrine, to treat the patient like a human being, but after the patient is under ether, those who are willing to follow Crile thus far fail to agree with him. In the first place, physiologists have been unable to produce a fatigue of the vasomotor centers after hours of peripheral sensory stimulation under ether, and secondly, it has been recently demonstrated by the use of the string galvanometer that afferent impulses do pass up the cord on sensory stimulation, but that they are abolished by ether anesthesia.[2]

[2] Forbes and Miller: Am. Jour. Physiol., 1916, xl.

I would define shock as a condition marked by a gradual progressive fall of blood-pressure with no obvious cause, such as hemorrhage, intracranial pressure or heart failure. I believe that it is due to a paresis or paralysis of the musculature of the arterioles. I am aware of the fact that many think otherwise. A recent writer says: "When the development of shock is carefully investigated it is apparent that the fall of arterial pressure is caused by diminution in the output of the heart. The fall is not due to the abolition of the peripheral resistance in the arterial system. Henderson and others have pointed out that no inhibition or fatigue of any sort occurs in the vasomotor system. On the contrary, this organism is intensely active in the effort to compensate the blood stream, nor is the heart itself weakened. When the pressure in the venous system is observed it becomes evident that the apparent cardiac failure is the result of diminution of the pressure and volume of the venous stream to the right heart. The etiological sequence in certain forms of shock at least appears to be venous stasis, cardiac failure and fall of arterial pressure."

I am ready to agree that the central vasomotor nervous system shows no evidence of failure and that the heart shows no weakness, but I still believe in a primary failure of the musculature of the arterioles for several reasons:

1. They are the only parts of the vascular apparatus capable, so far as we know, of being paralyzed. The idea that the veins are dilated other than passively, implies a mechanism which has never been demonstrated.

2. A dilatation of the arterioles would necessarily be expressed in the veins because the pressure of the heart would, by this dilatation, be allowed to pass directly into the veins. There would be no congestion or stagnation in the arterioles because there must always, as long as there is any circulation at all, be a greater pressure in the arterial side than in the venous side. In this connection the fact of anatomy might be recalled, that the arterial system is empty in the cadaver.

3. The fact that physiologists find the vasomotor centers intact proves only that the centers are intact. I think they are looking at the wrong end of the vasomotor system. Why do I not go to work and prove it? Because thus far I have seen no way of demonstrating my contention. We must at any rate remember that the point where the controlling force of the vasomotor center, the energy-producing mechanism, accomplishes work, is in the arteriole. The vasomotor center is the dynamo. The nerves form the transmission system. The arteriole is the motor which transforms the energy produced by the dynamo into work.

4. What I consider shock is a manifestly toxic condition. High obstruction, acute pancreatitis, severe burns, severe infections, —crush injuries—all of these are conditions marked by the poison-

ing of the body with the toxic products of protein break-down. I suspect that these toxins injure the adrenals and the adrenals I believe are concerned in the preservation of the tone of the muscle cells of the arterioles.

5. The only way in which I have been able to experimentally produce anything which looks like shock to me is by the removal of the adrenals. After the removal of the adrenals the content of the intestinal tract contains a poison identical with, or at least similar to, the poison found in high obstruction. This fact has appeared in the work of Dr. Hendrix and myself this winter, and suggests a possible relation between high obstruction and the adrenals.

6. The relation between psychic shock and traumatic shock is compatible with my ideas. The relation between fear and anger and the adrenals is capable of experimental proof. The relation between psychic shock and that profound disturbance of metabolism known as thyrotoxicosis is universally admitted.

And, finally, my ideas of shock are entirely compatible with the best and latest in treatment. Porter,[3] as a result of his studies of shock at the front, advises:

1. A special position of the wounded so that the abdominal vessels shall be higher than the heart and brain (in other words, counteract the effect of hydrostatic pressure).

2. Heat. (The use of heat in shock is not clear to me. It seems like the empirical combating of a symptom, but it is universally used in practice.)

3. Intravenous injections of saline solution.

4. Intravenous injections of epinephrin. (Both of these procedures support the peripheral mechanism. Adrenalin is certainly purely a chemical supporter of the peripheral mechanism.)

5. The transfusion of blood in certain cases.

6. The observation of the diastolic pressure every half-hour, as an index of the condition of the patient. (In other words, we are dealing with a disturbance of the blood-pressure. Therefore, keep track of the blood-pressure by the only possible means, and if it be found to be failing, apply the principles of preventive medicine.)

What is the relative value of saline infusion and transfusion of blood? When should the one or the other be used? An answer to these questions must be based on personal opinion rather than on experimental or clinical fact, I think, because the men who favor any given method seem to become soon obsessed with an enthusiasm which tends to cloud the accuracy of their observations. The impartial, accurate observer is a rare bird.

The operation of transfusion of blood has a very interesting history. There are records which seem to show that the operation was practised in very ancient times. A review of an ancient Jewish

[3] Boston Med. and Surg. Jour., 1916, clxxv, 854.

writing which was shown to La Martinière by Ben-Israel Manasse, a rabbi of the Jews of Amsterdam, contains the following words: Naam, prince of the army of Ben-Adad, king of Syria, being attacked with leprosy, resorted to physicians who, in order to cure him, withdrew blood from his veins and replaced it with other blood. Perhaps this was not a transfusion, but early serum therapy. At a much later day, Libavius, in his treatise of the sacrifices of the Emperor Julian, speaks of transfusion as having been an eye witness of an operation of this sort.

About the middle of the seventeenth century, following upon the discovery of the circulation of the blood by Harvey in the early part of this century, together with the then prevailing philosophical conjectures concerning the real function of the blood, the question of the transfusion of blood became of unusual interest. Serious discussions were held on points such as if the blood of a sheep were transfused into a man, would he grow wool and horns, and the treatment of incompatibility of temperament (or temper) of husband and wife by mutual transfusion was seriously proposed. In the year 1667 Denis, professor of philosophy and mathematics, at Paris, performed the first transfusion operation upon a human being, that is, the first operation the account of which we find preserved in an original record. In the fall of the same year Lower and King operated in London.

The German physiologist Landois, in 1875, collected 374 cases of transfusion of human blood, of which in 150 cases the result was favorable, in 180 unfavorable, doubtful in 12, while in 3 no result was to be expected, and 2 died during the course of the operation. He also collected 129 cases of transfusion of animal blood into the human body, of which 42 resulted in a cure or continued improvement, 25 in transitory improvement or doubtful success, and 62 were followed by no improvement and death. These figures are of little value, all dating from the preaseptic days, but I give them to you to show the extent of the practice of transfusion, especially remarkable in those days in which there was relatively little surgery practised. Further, these figures show that the operation has always had a strong hold on the imagination both of the public and the surgeon, and there are more recent indications that transfusion has more often appealed to the poetic sense than to the common sense.

Landois was the first apparently to study this problem, and he arrived at two important and fundamental conclusions: (1) He observed that the mixture of the blood of two different species results in the destruction of one or both bloods—in hemolysis—and it was this work which laid the foundation for all the subsequent work on hemolysis and bacteriolysis, with the discovery of all the methods of serum diagnosis now of such invaluable aid in clinical work. Landois further demonstrated that an animal dies of

hemorrhage at a time when there are still plenty of red cells left in the body to carry on all the functions of the red cells, but when the fluid column of the blood has fallen so low that the heart cannot get a grip on this fluid column; in other words, death from hemorrhage in the normal animal results from the mechanical disturbance of the heart action rather than from the loss of a chemically vital fluid. Landois found that death after hemorrhage could be prevented if this fluid column were restored by the addition of fluid, and he introduced the method of saline infusion for the treatment of hemorrhage and shock. So saline infusion completely displaced

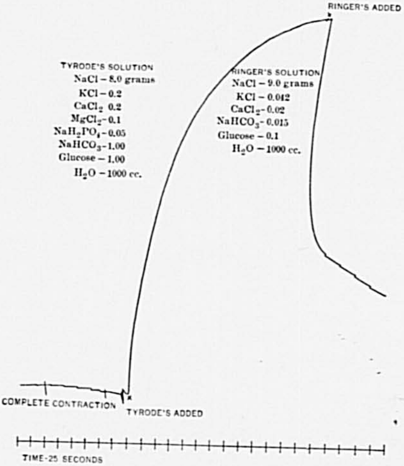

Fig. 1

blood transfusion for many years until transfusion was revived, and thus the pendulum swings backward and forward until after many swings through an arc, which gradually grows shorter and shorter, the pendulum stops at the exact center of the arc which represents the truth.

The demand for fluid by the body after hemorrhage is so insistent that fluid is taken up rapidly from the tissues, from the great serous cavities, from the stomach, intestines, the large intestine; the kidneys stop excreting. Therefore we make use of these facts in surgery. We give patients freely to drink; we inject saline into the tissues subcutaneously; we fill the large bowel with saline and

finally we inject saline directly into the veins. These methods are all of value and they differ only in the speed of attainment of the end-result.

When shall saline be used and when transfusion of blood? Now, salt solution has the great advantage of being always on hand in sterile form; a hollow needle, not too small, a rubber tube and a funnel, introducing the needle into a vein, directly through the skin—making an incision to expose the vein indicates a lack of refined technic—and the life-saving process is started. I personally firmly believe in the teaching of Landois. I feel that salt

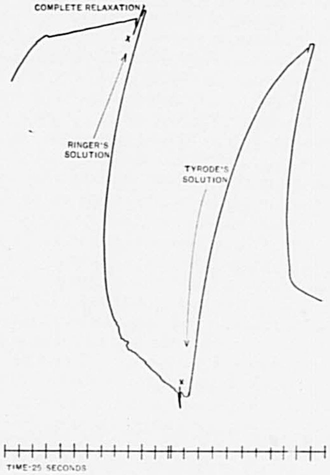

Fig. 2

solution does all that transfusion can do in the acute cases, perhaps not in the late anemias, and only two things stand out to the disadvantage of the salt solution. You may remember that I defined blood-pressure as the result of the friction of a viscid fluid in the small arteries of the periphery. Salt solution does not possess the normal blood viscosity, and it may be for this reason that it does not work sometimes. And, again, rarely, salt solution seems to leak through the lungs so rapidly as to cause acute edema. Some day I believe someone will find some colloidal addition to give saline a viscosity comparable to that of the blood, and this objection will then be done away with.

There is another point concerning salt solution which I believe worth mention. If we have learned anything from the recent work with the Dakin solution in the treatment of infected wounds, it is to my mind the fact of the importance of a surgeon's either knowing chemistry, or, since that seems to be such a disheartening task, having associated with him a real physiological chemist. Let me emphasize again the facts that I have endeavored to point out, that blood-pressure is the result of the factors found in the vessel wall, chiefly the smooth musculature, acting upon the factors found in the fluid within the vessel. A very interesting point in this relation is found in a study of the technic of using smooth muscle preparations for the testing of certain substances such as adrenalin. For this test a strip of smooth muscle is suspended in warm oxygenated Ringer's solution, and if the Ringer's solution be not exactly right all sorts of results may be expected. I have here, for example, the actual tracing made by a strip of longitudinal muscle from the small intestine of a dog which gave either complete relaxation or complete contraction, depending only on which two of Ringer's solutions was added. Therefore I venture to raise the question whether some of the untoward results obtained after saline infusion are due to the fault of the theory, or simply to the fact that the saline solution was made up by a combination of a trained nurse and a tablespoon instead of by a chemist who realized the sensitiveness of living tissue to slight changes of concentration.

Do I not then believe in transfusion of blood? I fear I am not what you would call an extreme enthusiast. Let us think for a moment of what the blood is and does and let us begin by getting out of our heads the idea that the blood is that vital life-giving fluid which can be cleaned by a spring tonic or which can become diseased with any result, from pimples on the face to cancer of the uterus. The blood is just about as vital as a bucket of water and just as full of life. It contains and carries to the tissues just what and only what the tissues put into it. It is a purely passive, inert, lifeless stream, carrying canal boats loaded with goods, the red cells, themselves again passive. The red cells are alive, yes, but only mildly so, and no more so than certain chemical agents, sponge-platinum for example. The white cells are alive in a more real sense, but they are not in the blood stream because the blood stream wants them; they were merely playing on the bank and fell in.

Please do not think I am casting aspersions on the blood. I have the highest respect for it as a common carrier, no respect at all for it as an active agent, endowed with a free will; and because I feel as I do toward the blood, I fail to see that a transfusion can ever be much more than a symptomatic treatment. You put in some fresh blood and it may do good for a little time, but I do not see how you have transferred with it any incentive to a diseased bone marrow, for instance, to brace up and lead a decent life.

One hears many favorable reports of transfusion, too many, however, like a case I recently saw. An old man, nearly seventy, suffered from a severe chronic anemia. He had several transfusions and improved somewhat. He was suffering from an extensive pyorrhea and finally had his teeth removed. He improved rather remarkably. Transfusions still were continued. Those who will may say the transfusions did it; those who will may say that the removal of the focus of chronic infection did it. All I am sure of is that the transfusions did not kill him; but after one of these transfusions he suffered a chill, and that suggests another reason why I am no enthusiast. The blood, though passive, is an extremely complex solution and suspension, and I for one have a most profound respect for any combination of physiological chemicals which require only the very slightest change to transform them from normal compounds to virulent toxins. I am frank to say that on theoretical grounds I am afraid of transfusion, and therefore consider it only an operation of last resort.

I know that there be many who will land merrily with both pedal extremities upon my ideas concerning transfusion. No doubt I am in a very small minority, a small band of wilful men. I nevertheless do not hesitate, for I feel sure that there are many who might welcome the moral support of such a minority report. I have heard within the last few weeks of two fatal cases of transfusion in the hands of good men. I would encourage the reporting of these failures of surgery, for when surgeons stop reporting successes and report only their failures, we will note an important advance. The art of surgery demands results, but the science of surgery demands the truth!

MELANOSARCOMA OF THE RECTUM, WITH THE REPORT OF A CASE.

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The rarity of melanotic tumor of the rectum is sufficiently attested by the fact that many surgeons of wide experience have never seen a case. The rather striking fact that the condition is not unusual in horses has long been known; but Ashton[1] in a treatise on diseases of the rectum and anus published in 1857 did not "know whether melanotic cancer of the rectum had been observed in man, though I have seen in the autopsy room several cases of melanotic

[1] On the Diseases, Injuries and Malformations of the Rectum and Anus, etc. London, 1857, 2d ed.